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me my ledgers to read the copy through press, but all the original books of observation and books of reduction will be left here. I shall not require them, for I have read and re-read all the parts not printed. I shall also take with me the ledgers containing the unprinted observations incorporated in the Catalogue, 1877, 1878, and part of 1879. Mr. Gill will therefore be entirely clear from all responsibility for my work. I have pushed on the work here to such an extent that it would have taken more than four years to have printed the observations now completed for press, at the highest pressure ever attained at the Cape. Of course the first thing is to print the Catalogue, which contains all the results. Probably it may afterwards be desirable to print at least the table of instrumental corrections, and the separate mean results for 1877, 1878, and 1879; but I should not think it would be worth the expense of printing the annual Catalogues for these years. In this way expense would be greatly reduced, and everything needful could be given to astronomers. The work as sent home is complete, except that all the constellations have not been put in.

On the Value of the Solar Parallax derived from Observations of Mars made at Ascension Island during the Opposition of 1877.

By D. Gill, Esq.

It is well known to the Fellows of the Society that the special object of the Ascension Expedition was to determine the solar parallax from observations of the diurnal parallax of *Mars*. The reductions are now so far completed that I can communicate the resulting parallax to the Society; and I have thought it better to do so without further delay, as some time must necessarily elapse before the full details can appear in the *Memoirs*.

The general plan of observation and treatment of the reductions has been described in the *Monthly Notices* for November 1878. According to that method, the places of the stars of comparison are regarded as absolutely known.

It was found, however, that the mutual distances of the comparison stars computed from the coordinates determined by meridian observations differed systematically from the same distances measured with the Heliometer according as the fainter star preceded or followed the brighter star. When the brighter star was preceding the computed distance always exceeded the measured distance, and *vice versâ*. In other words (regarding a $4\frac{1}{2}$-mag. star as the mean magnitude of clock stars), the R.A. of the fainter stars appeared to be too great.

In the *Monthly Notices* for December 1878 it was shown that there appeared to be a systematic difference of personal error depending on magnitude, between observations by Eye and Ear

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and observations recorded by Chronograph. Separating the results derived from Eye and Ear observations from those derived from chronographic registration, it was found that in the mean the former agreed perfectly with the Heliometer results, whilst the latter showed a still larger discordance, depending on magnitude, nearly proportional to the magnitude, and amounting to nearly $0''\cdot25$ of arc per magnitude, for stars from 4·5 to 8·5 magnitude.

Thanks to the energy and kindness of many astronomers, these results have received much independent confirmation.

The most satisfactory method yet followed appears to be to observe the transit of a star over half the wires with the full aperture, then interpose a wire screen and observe the star over the remaining wires; the difference of results, when reduced to the middle wire, is due to Personality. To eliminate error in the wire intervals and possible change of collimation, the operation is repeated in the reverse order—the screen being used with the first wires, the free aperture with the last. The faint diffraction lines formed by the screen are entirely invisible in an illuminated field; the star disk of a 6th-mag. star reduced thus to 8th mag. by a wire screen, is quite indistinguishable from an 8th-mag. star observed in the ordinary way.*

Experiments have been made in this way by Dr. Becker at Berlin, by Professor Lewis Bass at the Dudley Observatory, Albany, U.S., and at Leiden by Professor Bukhuyzen's Assistants, and, I believe, also at other Observatories.

The results, so far as I have received them, entirely confirm the results of the Heliometer observations, and show that the R.A. of faint stars observed by the chronographic method is always too great.

I think it is desirable, before publishing the Memoir on the Parallax in full detail, to wait for the results of observations already made or in progress in connection with this investigation of the effect of magnitude on the R.A.

I have, however, satisfied myself that no alterations which are possible in the adopted places of the comparison stars will produce any sensible effect on the resulting parallax; because, not only must these be very small, but they will be nearly equally distributed amongst morning and evening observations.

I do not, therefore, on that account further delay communication of the parallax obtained. The method of reduction is quite similar with that employed in my preliminary paper (*Monthly Notices* for November 1878), except that two additional terms have been introduced in order to examine the influence of errors in the assumed refraction east and west of the meridian.

If r denotes the tabular refraction for any particular distance,

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* I had recently the opportunity to test the experiment myself at Leiden, and I was entirely satisfied as to this point.

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then the true refraction, if the observation is made east of the meridian, is supposed to be

$$r+\frac{r}{100}m;$$

and, if the observation is made west of the meridian, is supposed to be

$$r+\frac{r}{100}m'.$$

These terms m and m' are carried throughout the whole computation, so that the final coefficients of m and m' in the result express the effect on the parallax of an error of *one per cent.* in the tabular constant of refraction both east and west of the meridian.

As stated in my preliminary paper, it was obvious, from the arrangement of the order of observation and the positions of the comparison stars, that the final effect of errors in refraction must be insensible, and so the result of rigid computation has proved.

The actual coefficients of m and m', in the final result are

$$-0{\cdot}0002\,m \qquad\qquad -0{\cdot}0002\,m';$$

so that an error of $2\frac{1}{2}$ per cent. in the refraction tables would be required to change the resulting parallax $\pm 0''{\cdot}001$.

It is therefore impossible to conceive any systematic error due to refraction in its ordinary sense. The Astronomer Royal has, however, called attention to a possible source of error of great importance due to atmospheric dispersion. At considerable altitudes a star is not a disk, but is elongated into a short vertical spectrum by atmospheric dispersion, and we do not see the upper and lower limbs of a planet, but their spectra. It is very easy to conceive that the effect of this may be to shift the apparent place of the planet upwards or downwards, and thus an error in the same sense as parallax may be produced.

Such an effect, however, will vary as the tangent of the zenith distance. Therefore, if we divide the observations of each morning and evening into two groups—observations at greater and smaller zenith distance—we may, by comparing the parallax resulting from the observations at smaller zenith distances with that from observations at greater zenith distances, ascertain whether the effect of atmospheric dispersion affects the parallax in a sensible degree.

Such an investigation accordingly was carried out. The small difference of the two resulting parallaxes ($0''{\cdot}006$) appears to show that the error due to atmospheric dispersion is insensible.

The observations before and after Opposition were also independently reduced, and the results agree within limits of probable error.

Finally, in order to test whether possible error in the star

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places could produce sensible error in the result, a final reduction was made in which only those observations were employed where the same stars were systematically employed the same evening and morning. The result is within the limits of probable error.

The assumed solar parallax was 8″·80.

The following are the direct results, where n is the number of $\frac{1}{100}$ parts which the assumed parallax has to be increased, according to the notation of the Astronomer Royal:—

	Value of n	Weight	Corresponding Solar Hor. Par. ″
From all observations combined	−0·189	5·20	8·783
„ observations at greater ZD's	−0·164	3·03	8·786
„ „ at smaller ZD's	−0·223	2·17	8·780
„ „ before opposition	−0·098	3·06	8·791
„ „ after „	−0·317	2·14	8·772
„ symmetrical observations only	−0·430	2·12	8·762

The weights given for the various results have been rigidly computed. The probable error corresponding to weight 1 is equivalent to the probable error of a single complete measure of distance.

If then

r = the probable error of 1 observation of distance,

w = the weight of any of the above values of n,

then the probable error of the resulting parallax will be

$$\frac{8\cdot8}{100} \times \frac{r}{\sqrt{w}}.$$

Until all means have been exhausted to reduce the errors of the star places, it is is not desirable to compute rigidly the final probable error. But if we adopt even so extreme a probable error as ±0″·40 for the single observation of distance (and there is good evidence that it is less than this), the resulting parallax from all the observations is

$$8''\cdot78 \pm 0''\cdot015.$$

It is interesting to remark that this result agrees with that derived from the combination of Struve's constant of aberration with Cornu's determination of the velocity of light.

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